Plankton Production Biology

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LONG-TERM GOALS

I continue to investigate geographic and seasonal distributions of hydrography and nutrients in respect to plankton production in the central and eastern Arabian Sea including the continental shelf of India west coast. This year I have added the central Bay of Bengal. Occasionally I comment on general oceanographic or ecological principles.

OBJECTIVES

The objectives during the reporting period continued to be old and new oceanographic issues about the central and eastern Arabian Sea including the Indian shelf, and now the central Bay of Bengal.

APPROACH

Largely, cost-efficient mining of old and new oceanographic data is used, some of which are not yet in data centers. The principal tools are vertical property plots for the upper 0.5 km of the water column, temperature-salinity diagrams, and regression analysis. The work is done with colleagues at India's National Institute of Oceanography in Dona Paula (Goa). I enjoy their trust, so observations from inside the Indian EEZ are available. As always, the accuracy of the data is of concern.

WORK COMPLETED

(A) ***Bias in early dissolved O_2 analyses especially for Oxygen Minimum Zones***

A principal biological and biogeochemical function of the oceans' Oxygen Minimum Zones (OMZs) is the reduction of nitrate when O_2 is lowered to 1-2 µM. It leads to formation of N_2 and N_2O. The former process removes the bound N from the sea, which is delivered perpetually by rivers, lightening, and fixation of N_2 by some phytoplankton. The latter is an effective greenhouse gas. The big current geochemical issue is whether the three major OMZs of the world's ocean intensify and expand (e.g., Stramma et al., 2008). Clearly, accurate time series are the ground truth.

The OMZ of the AS is the largest of the three major ones. As a spin-off from our ongoing study of the temporal stability of this OMZ we have discovered the role of a switch during the 1960s to 1980s in the method for endpoint detection for the Winkler analysis of dissolved O_2. Even with O_2 now

Report Documentation Page

Form Approved
OMB No. 0704-0188

1. REPORT DATE **30 SEP 2011**	2. REPORT TYPE	3. DATES COVERED **00-00-2011 to 00-00-2011**

4. TITLE AND SUBTITLE **Plankton Production Biology**	5a. CONTRACT NUMBER
	5b. GRANT NUMBER
	5c. PROGRAM ELEMENT NUMBER
6. AUTHOR(S)	5d. PROJECT NUMBER
	5e. TASK NUMBER
	5f. WORK UNIT NUMBER
7. PERFORMING ORGANIZATION NAME(S) AND ADDRESS(ES) **University of Washington,Applied Physics Laboratory,1013 NE 40th St,Seattle,WA,98105**	8. PERFORMING ORGANIZATION REPORT NUMBER
9. SPONSORING/MONITORING AGENCY NAME(S) AND ADDRESS(ES)	10. SPONSOR/MONITOR'S ACRONYM(S)
	11. SPONSOR/MONITOR'S REPORT NUMBER(S)

12. DISTRIBUTION/AVAILABILITY STATEMENT
Approved for public release; distribution unlimited

13. SUPPLEMENTARY NOTES

14. ABSTRACT

15. SUBJECT TERMS

16. SECURITY CLASSIFICATION OF:			17. LIMITATION OF ABSTRACT	18. NUMBER OF PAGES	19a. NAME OF RESPONSIBLE PERSON
a. REPORT **unclassified**	b. ABSTRACT **unclassified**	c. THIS PAGE **unclassified**	**Same as Report (SAR)**	**6**	

Standard Form 298 (Rev. 8-98)
Prescribed by ANSI Std Z39-18

measured with electrodes routinely and continuously on stations, on Argo floats, etc., the reference standard remains the Winkler titration of water samples.

We submitted a somewhat long manuscript about the associated issues to *Geophysical Research Letters* (Banse et al., 2011) with the following amended Abstract.

"At the very low O_2 concentrations in oxygen minimum zones, O_2 measurements with the traditional Winkler method using starch as indicator for visual detection of the titration end point tend to be too high by ~ 0.05 up to 0.10 mL L^{-1} (~ 2-4 µM), a large bias relative to the median O_2 levels in OMZs. In the last two to three decades, automated potentiometric or amperometric approaches have taken over. Data centers do not flag the biased values. Using them unknowingly in oceanic time O_2 series extending back to the 1960s, the usual starting date for such studies, the possibility of falsely finding declines in O_2 is real and of ocean-wide significance. Also we note that comparing O_2 with NO_2^- in the same samples from OMZs can recognize faulty O_2 data, as well as be used to correct for the overestimate of apparent O_2 by the NO_2^- interference. In new field work, this interference should be eliminated by azide addition to one of the Winkler reagents, as is the rule in freshwater work."

The manuscript was not accepted, largely because of reviewing too much. In the meantime, Wong (2011) submitted a paper to *Marine Chemistry* addressing fully our observations about NO_2^-. We plan to publish the other, more important part of our manuscript, which is the role of un-recognized bias in O_2 time series for low-oxygen regions for the 1960s and 1970s, and in places the 1980s.

(B) **Highlights of *Notes and Questions by an Onlooker***

Six single-spaced pages plus figures for a white-paper book (in preparation) supporting the meeting of the **OCB - Global Ocean Flux Observatory – Ocean Observatories Initiative** (23-25 May 2011 at the Woods Hole Oceanographic Institution)

The comments were provoked by the titles of the lectures in the program, which indicated a bias toward the old view of studying fluxes as principally bottom-up regulated processes (i.e., resource-related). Instead, fluxes into or out of ecological compartments result from the shifting balances between gains and losses, e.g., of rates of cell division and mortality.

For advancing the knowledge of the euphotic zone a crucial task is a mechanistic understanding of the concentration of phytoplankton and its rate of change. The latter is far smaller than ^{14}C (uptake) and even cell-division rate (both under bottom-up control). The rate of concentration change may not even have the same sign as the two named rates because of the top-down effects of grazing and predation mortality which control quality and quantity of phyto- and zooplankton. Mortality in the field, however, is very difficult to estimate, leave alone to measure, except for micro- and picozooplankton.

The zooplankton rate processes are the big challenge. We have not invested enough in determining basic rate processes by laboratory experiments with common species. In my opinion, therefore, at present we have little hope for predicting from environmental data with useful accuracy for particular situations what percentage of phytoplankton production will not leave the euphotic zone because of being re-mineralized within it. An estimate of "80 or 90%" means a twofold difference of flux for the carbon budgets at depth! Maybe, a shift in the organization of research is needed?

For the meso- and bathypelagic zones, remotely identifying and sizing, or even catching the zooplankters without rate measurements do not suffice for balancing the carbon budget driven from above by the biological pump. Until the current imbalances in our budgets of these regions are overcome, measurements by traps, etc., of vertical flux cannot tell much by themselves. Where do we have all three terms of "Flux = Concentration x Sinking Rates" studied at the same site and time? Without that, we underdetermine the systems and cannot check the accuracy of our field data. And what about the roles of archaea and heterotrophic bacteria in the meso- and bathypelagic budgets?

(C) ***The central Bay of Bengal as an estuary - why is there next to no surface nitrate in spite of much entrainment from below?***

Extended summary of nine single-spaced pages, plus many figures, of a Memo to the SIBER Scientific Steering Committee [**S**ustained **I**ndian **O**cean **B**iogeochemistry and **E**cosystem **R**esearch]

The levels of ^{14}C uptake by phytoplankton and of chlorophyll in the central Bay of Bengal are clearly lower than in the offshore Arabian Sea (Prasanna Kumar et al., 2010), largely due to the different atmosphere-ocean interactions and the resulting stratification.Yet, years of deployment of particle interceptor traps at around 1 km depth show fluxes of particulate organic carbon (POC) quite comparable to those in the central Arabian Sea (Unger et al., 2003, Stoll et al., 2007). The nutrient contributions by rivers and the atmosphere appear to be relatively small (e.g., Rao et al., 1994; Sanjeev Kumar et al., 2004), and so the POC is principally of marine origin (Unger et al., 2005).

The Bay as a whole functions as an estuary driven by the large sum of river runoff plus rain minus evaporation. Low-salinity water with next to no nitrate flows out at the surface, while high-salinity, high-nitrate water moves in at depth and is entrained into the upper layers. In the middle of the Bay, a simple annual circulation model would consist of a surface layer of several ten meters depth with 25-30˚C temperature, 32.5 to 34.5 psu, and 0.1 up to perhaps 0.3 µM NO_3^-, while about 35.0 psu and 30-40 µM NO_3^- prevail below a distinct pycnocline. An upper-layer salinity of 32.5 is maintained by mixing of one volume of freshwater with 13 volumes of 35.0 psu water (32.5 divided by [35.0 - 32.5 = 2.5]). This will entrain 27-37 µM NO_3^- into each liter of the surface layer. The "dilution ratio" is even larger when considering a seasonal surface salinity of, say, 33.5. In fact, however, year-round we observe the water above the pycnocline having near-zero NO_3^- concentrations except sometimes during winter in the north and in the fairly common cyclonic eddies when they penetrate into the mixed layer.

Regardless of the mechanism transporting the NO_3^- upward, only the salt remains above the pycnocline. What happens to the entrained NO_3^-? Aside from the transitory blooms caused by eddies and their subsequent disposition, is the principal reason the deep chlorophyll maximum (DCM), which traps the NO_3^- moving upward in the water that maintains the salt balance of the surface layer (e.g., model by Jamart et al., 1977, Fig.13). The mechanism is likely at work in all non-transient DCMs including those of large lakes. A field study in the Bay of Bengal would uniquely benefit from the high initial NO_3^- values and, especially, the use of the salt balance for determining vertical flux, rather than the necessarily imprecise physical estimates of eddy diffusion and vertical advection.

To address the issue I have sketched a large integrated research program to (the IMBER-approved) SIBER. The overall goal would be a nutrient budget of the central Bay of Bengal treated as an estuary. If studying at first only the processes in the DCM, the essence would be to compare the upward flux of NO_3^- through the pycnocline, as determined from the salt balance, with the measured NO_3^- uptake in

the DCM over suitable time scales. I have summarized details of needed measurements for SIBER's Scientific Steering Committee.

(D) ***Translations of monographs from the Russian***

Scientists in the "West" are largely ignorant about Soviet and Russian-Ukrainian oceanographic publications unless they appear in the few journals translated from cover to cover. To open windows, therefore, since the mid-1990s in some of my grants ONR had supported translations of five monographs and the commission of a new book in English. The financial and logistic problems of editing and printing the manuscripts has led to very long delays, but three were published in 2006 (Banse and Piontkowski; Pavlova; and Sazhina). During FY2011 the editing of a fourth monograph, by Sazhina (1985) was almost finished (together with Senior Lecturer Dr. Andrew G. Hirst of the University of London, Queen Mary). Remaining is the partial renumbering of plates and captions and the preparation of the entire manuscript before submission.

Sazhina's book of 1985 provides **illustrated keys for the six stages of the nauplii (copepod larvae) of 85 species** broadly distributed in the Atlantic with its adjacent seas, the eastern tropical Pacific, and the warm parts of the Indian Ocean. It is the first and after 26 years still the only one key of its kind. It will permit the study of stage-specific population dynamics (growth rate, production, mortality) of copepod larvae in mixed populations. Since biology proceeds through species rather than carbon and chlorophyll, the work once available, will be invaluable.

We intend to submit the manuscript for printing by India's National Institute of Oceanography (N.I.O.) in Dona Paula, Goa, toward free electronic distribution. The N.I.O. director, Dr. S.R. Shetye, has encouraged the preparation of translations of monographs, which cannot expect commercial production in the west or, as possibly with this one, resulting in a high sales price making the book practically inaccessible to users in developing countries.

Two monographs about comprehensive Soviet-Ukrainian expeditions to the same two station nets ("polygons", squares or rectangles of several tens of stations, generally with half-degree spacing) in the equatorial Indian Ocean in 1980 and 1985, and another cruise in 1983 (Petipa, 1986, 1993) have been translated but require much editing, which I have delayed because of the problems in finding outlet for publication. Translated hydrographic chapters about the divergence near 10˚S, though, were sent to Indian and French colleagues.

IMPACTS and IMPLICATIONS

1. The recognition of bias in historic O_2 analyses at low concentrations as in OMZs should lead to checking of the important geochemical conclusions about intensification and expansion at the few sites where time series date back to the 1960s and 1970s.

2. Memo to a bottom-up oriented workshop: We must treat concentration changes as the balance between processes of gains and losses of biological processes. Only the gains are resource-related. (Old professors - this one being 82 years of age - tend to tell the same old things over and over, lest some facts, published a long time ago, not be recognized by the young generations.)

3. The memo about the Bay of Bengal addresses Indian as well as American and European colleagues. Especially the non-Indian scientists are abandoning the Arabian Sea because of the

Somali pirates and the resulting high premiums required for insuring our ships. It is to be seen whether my message will be picked up.

4. The translation and distribution of Sazhina's keys will permit the study of stage-specific population dynamics (growth rate, production, mortality) of copepod larvae in mixed populations as in the field or captured water columns (mesocosms).

RELATED PROJECTS

I continue to stay in contact with India's National Institute of Oceanography in Dona Paula, Goa, and occasionally advise the director.

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